all his spare time was devoted to the committing of Euclid to memory! I shuddered as I thought of what was to be my own fate in a few short months, when I too must be subjected to this fearful imposition. But the first hour or two which Dr. Gloag (a name strange, perhaps, to southern ears, but very high indeed on the roll of successful teachers—Clerk-Maxwell, indeed, was one of his pupils) devoted to geometry showed those of us who had any taste for the subject that it was one to be learned by head, not "by heart" (the idiotic phrase in common use)—and that my friend's parents had simply taken him from a good teacher and sent him to an exceedingly bad one—for it came to be discovered after some time that he had really considerable aptitude for geometry.

But if he had been in fact quite unfit for the study, otherwise than in learning to repeat Euclid by rote, what object beyond mere torture would have been attained by forcing it upon him? This leads to another remark of great importance in connection with the mass of elementary text-books.

What sort of students are those who require to be told to *take the square of the velocity, divide it by the radius, and find the proportion of this quotient to* 32 . . . :—without farther explanation or proof? What the better are they of the information? Call you this "teaching science?" Has it improved their minds? Will they be able to make any use of it in after life? I do not see how these questions and many other connected ones can be answered except by a prompt negative. One of two things. The pupil who requires to be taught in this way is either as yet too young, or is one who will never become old enough, to learn even the rudiments of science.

To our metaphor once more. Grass-plats, moss, and flower-beds for the happy sports of children—the bare rock and rough moor for the stern work of men. Your gravel-walks and Macadamised roads are excellent things in their way, but keep them to their legitimate users, the carriage and the perambulator for the invalid and the infant who can neither work nor even play.

My reasons for writing on this subject are very serious ones. I have to consider each year how best to instruct some couple of hundred students in the elements of physics, and have to be constantly on the out-look for a really good text-book of an elementary character. In the higher branches of the subject there is, happily, little difficulty, but that a really good, short, and simple treatise on the merest elements has been (at least till very lately) wholly unprovided is, I think, clear from the ridiculous discussions about *Centrifugal Force*, and other connected ideas, which are even now constantly to be found in our more practical periodicals.

P. G. TAIT

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Nectar-Secreting Glands

MR. FRANCIS DARWIN has made an interesting addition to his important discovery of nectar-bearing glands on the young fronds of *Pteris aquilina*, supplied from the ever-welcome experience of Mr. Fritz Müller. The latter gentleman finds that in Brazil the *Pteris aquilina* is protected from the leaf-cutting ants by those attracted to the nectar, and Mr. Darwin adds some speculations on the origin of the glands and their continued functional activity in Europe where they now appear to be useless. On this part of the question I should like to make the following remarks:—

Prof. Heer has shown that in the Miocene plant-beds at Œningen and Radoboj, ants are the most numerous amongst the fossil insects, and in 1849 as many as sixty-six species had been described from these two localities. In 1865 the number found at Œningen alone is recorded as forty-four. I do not know what the total number of species is that have been recorded from the two places up to the present time, but it probably does not fall short of eighty. Amongst the fossil ants from Radoboj there are species of the Tropical American genera *Atta* and *Ponera*. One of the fossil species of *Atta* resembles in general form and in the venation of the wings the curious *Atta cephalotes* of Tropical America.

As there are only about forty species of ants existing now in the whole of Europe it is evident that in the Miocene epoch they must have played a much more important part in Europe than they do now. Plants may then have been exposed to the attacks of enemies that have become extinct along with the general impoverishment of the fauna and flora of Europe that took place in Post-pliocene times; and the protection afforded by ants attracted to the nectar-bearing glands at the critical stage of the unfolding of the young and tender leaves may have been as important to some plants in Europe, then, as it is to many in Tropical America now.

With regard to the persistency of the nectar-producing glands up to the present time in Europe, it is to be remarked that many plants are identical with those living in the Miocene period and the world-wide distribution of *Pteris aquilina* seems to indicate that it is of very ancient origin. If a plant has not otherwise varied there is no reason apparent why it should do so in this respect so long as the secretion of nectar is not positively injurious to it. I have recently noticed in my garden that the ants that attend the glands at the bases of the leaves of the cherry, the plum, the peach, and the apricot, stroke with their antennæ some of the glands that are not excreting when they arrive at them, just as they do the bodies of the aphides. I have not actually noticed that this promotes a flow of nectar, but ever since I became a disciple of Darwin I have been convinced that the most trivial circumstance is worthy of notice; and it may be that the slight irritation of the glands kept up by the ants is sufficient to ensure the perpetuation of a function of the plant now useless to itself. It is, however, perhaps too soon to assume that the glands are entirely useless to the plants in Europe. Darwin states that there is good evidence that the absence of glands in the leaves of peaches, nectarines, and apricots leads to mildew ("Animals and Plants under Domestication," vol. ii. p. 231).

Darwin refers at the same place to the variation of the glands of the leaves in the above-mentioned fruit trees and I may add that they are extremely variable on the cherry, being sometimes absent, sometimes on the stalk and sometimes on the blade of the leaf. The young leaf in its earliest stage, before it expands, has a complete fringe of them, thus bearing out Mr. Francis Darwin's theory that they are homologous with the serration-glands of Reinke.

May I suggest to some of your correspondents that information as to how far north in Great Britain or in Europe the glands on the above fruit trees are attended by ants and especially if the wild cherry (which I have not had an opportunity of observing) is so attended, would be of great interest.

THOMAS BELT

Cornwall House, Ealing, June 8

On Time

"The fact is, that we have not yet quite cast off the tendency to so-called metaphysics."—Tait, "Rec. Adv. in Phys. Sc.," p. 11.

IN Thomson and Tait's "Natural Philosophy," of which I have only the German edition in my possession, I find, § 246: "Die Zeiten, während welcher irgend ein besonderer Körper, der durch keine Kraft angetrieben wird, die Geschwindigkeit seiner Bewegung zu ändern, gleiche Wege durchläuft, sind einander gleich." And § 247: "Dieser Satz drückt bloss die für die Messung der Zeit allgemein getroffene Uebereinkunft aus."

These quotations quite express what is generally understood.

Yet in the definition of the equality of two lapses of time there is a logical fault. It is not allowed implicitly to introduce in a definition what is to be defined. There is no body of which we know *à priori* that no force tries to alter its velocity; in order to ascertain this, we must find out in consistency with the usual definition of force, given in § 217, whether it moves through equal spaces in equal times.

The definition of § 246, therefore, really says: The times, in which a body that goes through equal spaces in equal times moves through equal spaces are equal. It is evident that we are reasoning in a circle.

I am very well aware of the objection which will be made. We have it in § 245: "Auch werden wir später sehen, dass ein vollkommen glatter sphärischer Körper, welcher aus concentrischen Schalen besteht, deren jede von gleichförmiger Material und überall von derselben Dichtigkeit ist, sich, wenn man ihn in eine Drehung um eine Axe versetzt hat, trotz hinzutretender einwirkender Kräfte mit gleichförmiger Winkelgeschwindigkeit dreht, und seine Rotationsaxe in einer absolut festen Richtung erhällt." Thereupon it is said in § 247 that the earth is a body which fulfils these conditions very nearly, and that therefore its rotation gives us the means to measure time. But this assertion is not at all proved.

I now request my readers to be so good as to follow the exposition of *my* view. I assume that we are able to decide whether two lapses of time are equal. How this is done I shall dwell on afterwards.

When the conception of time is combined with the conception of motion we arrive at the building up of kinematics, in which the ideas of velocity and of acceleration are introduced. In abstract dynamics the idea of force is first introduced, wholly separated from any definite physical sense. As soon as the state of motion of a body (which is determined by the magnitude and the direction of its velocity) undergoes a change, we think of a cause of this change, and call this cause a force. We ascribe to a force magnitude and direction. If a body, which primarily is in rest, acquire a rectilinear motion, the force has constant direction. The magnitude of a force of constant direction is judged by the increase of velocity, which it gives in a definite time to a body primarily in rest. If the increases of velocity in equal times be equal, the force has constant magnitude. Two forces of constant direction and magnitude are in the same proportion as the increases of velocity which they give in equal times to the same body. Unity of force is the force which in unity of time gives to a particular body unity of increase of velocity.

It is conceivable that equal forces acting on different bodies cause different accelerations. Therefore another idea is introduced—the idea of mass. It is settled by definition that the masses of two bodies are in inverse proportion to the accelerations which they receive from equal forces. To a particular body unity of mass is ascribed. Unity of force is the force giving to unity of mass unity of acceleration.

I need not dwell on other ideas which are introduced, *e.g.*, moment, work, energy, &c. The whole building can be constructed, and there is room for every investigation which belongs to so-called theoretical mechanics. So it is demonstrated that a centrobaric body, which has kinetical symmetry in respect to its centre of gravity, and which has been brought in rotation about an axis going through the centre of gravity, retains constant angular velocity, when no forces are acting on the surface, and on the component parts only central forces which are in the same proportion as the masses of the parts.

Before kinematics and abstract dynamics can be applied in interpreting phenomena, we must be enabled to measure time.

What is time? There are mental conceptions which cannot be described by words, and I reckon "time" amongst them. But I shall try to answer the question how the conception of time originates with us.

The formation of the conception of "time" is preceded by the formation of the idea of "lapse of time." The idea of "lapse of time" we arrive at by the simultaneous observation of two phenomena, in conjunction with the observation of two phenomena not occurring simultaneously, in such a manner that we receive the impression of the second phenomenon when the impression of the first one is not yet effaced from our memory.

A lapse of time, from the nature of the idea, is limited. If we abstract the *definite* limits, we have the conception of time.

It is clear that in speaking of the measuring of time we properly mean the measuring of lapses of time.

In order to measure lapses of time we must know when a lapse of time is twice as long as another. We easily come to this on its having been established which lapses of time are equal.

If we wish to compare the length of two bodies we place the one beside the other, or if circumstances prevent us from doing so, we successively place a third object beside each of them.

For the comparison of two lapses of time we lack such means and have to follow another way.

In nature phenomena present themselves that persistently return. *Now we simply settle by definition that the lapses of time between the first occurrence of a particular phenomenon and the second is equal to that between the second and third occurrence.* Which phenomenon is to be chosen? Flux and reflux? Earthquakes? For the application of kinematics and abstract dynamics in interpreting phenomena, the choice is no indifferent matter.

I confine myself to the phenomenon which is still the usual base of the measurement of time. It is settled by definition that the lapses of time between the successive culminations of a definite fixed star in a definite place are equal. To divide these lapses of time themselves into equal parts, it is settled that the apparent motion of the fixed star, and therefore of all fixed stars, is uniform.

The results arrived at in the attempts at interpreting phenomena show that a very good hit has been made. But it is not impossible that after greater development of science we may have to make the measurement of time independent of the rotation of the earth. The application of abstract dynamics to the theory of the motion of the earth round the sun and of the moon round the earth has furnished admirable results. But in comparing the results of calculation with the accounts of eclipses found in ancient chronicles, a difference is met with, and in the opinion of some it is too considerable to be accounted for by the imperfection which may adhere to ancient descriptions. Therefore the theory of the motion of the earth and of the moon is incomplete. But hitherto no omission can be pointed out. For this reason some men of science are inclined to *settle by definition* that the theory of the motion of the earth and of the moon is complete, and to make it the base of the measurement of time. Then, of course, the former definition must be abandoned, and two arbitrary intervals between successive culminations of a fixed star no longer are equal.

Prof. Clerk Maxwell says ("Theory of Heat," second edition, p. 81): "This shows that time, though we conceive it merely as the succession of our states of consciousness, is capable of measurement, independently, not only of our mental states, but of any particular phenomenon whatever." In my opinion this assertion is erroneous. If we reject the rotation of the earth as the base of the measurement of time, we must have recourse to the motion of the earth round the sun or to that of the moon round the earth, or to any other phenomenon. Thomson and Tait, in § 406, already allude to a metal spring oscillating *in vacuo*. It should then be *settled by definition*, for example, that such a spring has a harmonical motion. If we proclaimed the lapses of time between the successive arrivals of flux and reflux at a particular station to be equal, and if we admitted, in order to divide these lapses of time into equal parts, *e.g.*, that the water sinks and rises uniformly, *then the whole of kinematics and abstract dynamics would retain the same form;* even then a centrobaric body with kinetical symmetry in respect to its centre of gravity, would show the peculiarity already mentioned. But it would be seen that our kinematics and abstract dynamics were but a highly deficient aid for the interpretation of phenomena; and the earth would not at all be a body with the same motion round its axis, as if it were a centrobaric body with kinetical symmetry in respect to its centre of gravity.

Of course it is wise to maintain provisionally the definition by which the earth in equal times rotates through equal angles.

In applying abstract dynamics to the interpretation of phenomena, we are led to identify the idea of mass with the idea of quantity of matter, and this has furnished excellently satisfying results. From this, in conjunction with experiment, it follows that two bodies which have equal weight, possess equal quantities of matter; that no matter is annihilated or created, &c.

This article is already too long for me to dwell on other consequences which follow from my view. Only a few words on the conservation of energy. This law threatens to be considered an axiom. Yet I believe it desirable that we should always remember that it is the result of experiment. If the measuring of time were founded on a different basis, it would not hold. Still the experiments do not give perfectly satisfying results. Usually this is ascribed to the imperfection of our methods and instru-

ments, which really may be the cause. But, probably, if in future times it be found by improved methods and instruments that the law does not hold, it would be advantageous to proclaim by definition the conservation of energy and to deduce from it the measurement of time. Then we should have the analogon of the absolute scale of temperature of Thomson.

If any one after the perusal of this article asserts that my views are at variance with the historical development of science, I answer that often in the reasoning of man there are gaps, which by contemporaries are not perceived; but that we must try to find them out and to fill them.

I hope my readers will not be too much annoyed by the defective manner in which I may have expressed myself in English; it is always difficult to make use of a foreign language.

Before closing I am bound to state that I have particularly mentioned the assertions of Sir W. Thomson, Prof. Tait, and Prof. Clerk Maxwell, because in their works I found most emphatically stated what in my opinion is erroneous. These eminent men stand so high that it is unnecessary for me to express my profound respect for them. V. A. JULIUS

Roermond, Holland

OUR ASTRONOMICAL COLUMN

THE D'ANGOS COMET OF 1784.—Encke's investigation relating to this reported comet appears in Zach's *Correspondance Astronomique*, as an "Imposture Astronomique grossière du Chevalier D'Angos, dévoilée par J. F. Encke, à Gotha." Olbers, in a letter addressed to him, had, as already stated, asked his attention to the subject, saying, "I would invite you to the examination of a doubtful comet of which the result will be either the knowledge of the yet unknown orbit of a very remarkable comet, or the discovery of a most shameful imposture," and adding particulars to which allusion is made in our previous note.

Encke remarks at the outset that, contrary to all general usage amongst astronomers, D'Angos had given the Malta observations with mean times for Paris, and the comet's positions expressed in longitudes and latitudes, which confirmed the suspicion that he had computed from the elements of an imaginary orbit and had not taken the trouble to convert the results into right ascensions and declinations, in which astronomers are accustomed to present them. If it is demonstrable that according to the observations (at least without supposing them erroneous to the amount of many minutes) the comet could only have moved in a very improbable orbit, in fact almost as a satellite of the earth and at a distance less than that of the moon, and if further it can be shown that by a very simple error of calculation D'Angos was misled in deducing the places of the comet from the imaginary elements, then, Encke urged, there remains no longer the smallest doubt that he had invented all these observations. Making use of the positions given for April 15, 22, and 29, Encke assuming arbitrarily a value of the comet's curtate distance from the earth at the first date, finds the corresponding value for the same at the third date in order to represent precisely the longitude on April 22, and compares with the corresponding latitude. Thus if the curtate distance on April 22 be taken as 0·42 (we somewhat contract Encke's figures) the third distance is 0·55, the error on the middle latitude, − 16′, and the resulting conic section is a hyperbola; the same form of orbit is deduced when the comet's distance on April 22 is diminished to 0·25. If this distance be further diminished to 0·146, the orbit becomes an ellipse, but the error on the middle latitude is still − 12′·8, and it was found necessary to reduce the curtate distance to 0·00126 in order to represent this latitude with no greater error than − 2′·5; the resulting orbit being also an ellipse. Taking the solar parallax at 8″·86, this distance corresponds to 116,000 miles, or about half the moon's distance from the earth, and under the condition named above, on April 29 it would still be less than 160,000 miles. Thus Encke found it was necessary to assume the comet's distance from the earth, almost incredibly small if the errors of calculation are to be brought within the limit assigned by D'Angos to the differences between the places computed from his elements and his observations, or about 1¼′; and, he continues, a celestial body under such circumstances remaining for so long a time in immediate proximity to the earth, would assuredly have been retained within its sphere of activity, and D'Angos if he were the first would certainly not have been the only observer of this second moon. Rejecting then as beyond probability the conclusions necessarily drawn from an investigation in the manner here briefly described, Encke proceeded to examine the calculation of geocentric longitudes and latitudes of the comet from the elements assigned by D'Angos.

Taking for example the observation of April 15, the logarithm of the radius-vector calculated from the orbit of D'Angos is found to be 9·8208333, and continuing the computation the resulting position differs from the observation 47° in longitude and 15° in latitude, but supposing that by an error of the pen D'Angos had used a log. radius-vector ten times greater, or 0·8208333, with the same heliocentric longitude and latitude, the errors are reduced to 56 seconds and 34 seconds respectively, and making the same change in the log. radii-vectores at the other dates of observation, Encke arrived at the extraordinary result that the whole of the reputedly-observed places were represented within about the limit of error mentioned by D'Angos, and he insisted that with such proof there could be no possible doubt that the observations and the orbit of the comet, "ne soient entièrement fausses et controuvées, et que par conséquent il faut les rayer de tous nos catalogues des comètes, comme un astre chimerique qui n'a jamais existé;" while at the same time he acknowledged himself ready to do justice to the accused and to make the most ample satisfaction if he could be opposed by arguments as strong and apparently conclusive as those upon which he had founded an adverse verdict.

THE TOTAL SOLAR ECLIPSE, 1889, DECEMBER 22.—In continuation of notices of future total eclipses of the sun which have appeared in this column, the elements of the eclipse of 1889, December 22, are subjoined:—

G.M.T. of Conjunction in R.A. Dec. 22, at 0h. 24m. 50s.

	° ′ ″
R.A.	271 0 10·4
Moon's hourly motion in R.A. ...	41 20·7
Sun's ,, ,, ,, ...	2 46·6
Moon's declination	23 14 1·8 S.
Sun's ,, ...	23 27 10·3 S.
Moon's hourly motion in decl. ...	3 4·2 S.
Sun's ,, ,, ,, ...	0 1·1 N.
Moon's horizontal parallax	61 17·1
Sun's ,, ,, ...	9·1
Moon's semi-diameter	16 42·0
Sun's ,, ...	16 16·1

The central and total eclipse commences in long. 78° 52′ W., lat. 15° 22′ N., and ends in long. 60° 55′ E. and lat. 6° 53′ N., and it occurs with the sun on the meridian in long. 6° 27′ W., lat. 11° 5′ S.

At a point on the coast of Africa in 10° 6′ S. lat., totality commences at 2h. 8m. 55s. local time, and continues 3m. 34s. At Bridgetown, Barbadoes, totality begins at 6h. 47m. 6s. A.M. local time, and continues 1m. 48s., but the sun's altitude is only 6°. The following are points upon the central line, which will show that with a fair duration where the sun is near the meridian, the course of the eclipse is not a favourable one for observation:—

Long.	Lat.	Long.	Lat.
° ′	° ′	° ′	° ′
59 22 W.	13 30 N.	5 5 E.	11 23 S.
47 39	7 37 N.	14 0	9 45
32 28 W.	0 23 S.	18 32 E.	8 27 S.